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II.—ON RECENT INVESTIGATIONS OF GRIMM'S LAW.

Ever since its discovery, more than fifty years ago, Grimm's Law has been the constant subject of discussions and investigations. Its bibliography will compare in extent with that of the Nibelungen and of other much mooted, perhaps never to be settled questions. Yet Grimm's Law differs from these. It is a generalization, based upon certain facts, sufficiently recognized by Rask and Grimm for the establishment of a principle, but not sufficiently understood and collocated and weighed by them to have made further investigation superfluous even soon after the discovery. What shall we say then of to-day, when the methods of investigation have been so greatly improved? There is a class of younger philologists in Germany and England who have so revolutionized the traditional methods, that they have received the name of the 'new school,' 'junggrammatische Schule.' Their principles have been repeatedly set forth and defended, *e. g.* in the review of Scherer by Paul in the *Jenaer Literaturzeitung*, No. 22, 1879, in Paul und Braune's *Beiträge*, IV, 315, and VI, 1. In full sympathy with Brugman, Joh. Schmidt, Osthoff, Sievers, Verner, Paul and Braune in Germany, are Sweet, Nicol, Murray and others in England. They do not believe, as Mr. Ellis expresses it, that philology is mere 'radicarian linguistry,' or the philologist, according to Mr. Nicol, a sort of 'cross between an antiquary and a postagestamp-collector.' They hold that to understand prehistoric speech-forms we must start with the historical and living ones; that phonetic laws are as free from exceptions as physical and chemical laws; that physiological and psychological processes must be kept strictly apart. Free play is given to accent, analogy and form-association; 'false' analogy, the men of the old school like to call it. For letter-comparison is substituted sound-comparison, pronunciation for orthography, the thing symbolized for the symbol. Phonetics, *lautphysiologie*, plays a prominent part in the usual phonology, *lautlehre*. We hear now of a history of sounds and of pronunciation, formerly only of historical grammar and history of literature. It is my purpose to discuss in a paper, or two, what progress has been made in the study of Grimm's Law by these new methods, what some of the problems are that

remain to be solved; and if I sift the literature on the subject as far as I may, some of the readers of this Journal may thank me for it.

Grimm's Law consists in a shifting of the mutes, as follows (*s. Geschichte der deutschen Sprache*, p. 276): the sonants shift to surds, the surds to aspirates, the aspirates to sonants. This he considers a real circular movement, all three shiftings going on at the same time. The languages concerned are 1. Any member of the Indo-European family except Teutonic; 2. Any Teutonic except High German; 3. High German. I shall try to restate and illustrate the law in a way less open to objection, I hope, than the old one just given.

Let y represent the sonant stop and z the surd one. These two are tolerably fixed quantities. x shall be that very uncertain quantity, generally called 'aspirate,' which is as objectionable a term as 'hard' and 'soft' for surd and sonant. The three groups of languages should not be—1. Greek or Latin or Sanskrit; 2. Gothic or English or Low German; 3. High German. For the first group Parentspeech (Prsp.), for the second General Teutonic (G. T.) are the terms most in accordance with the facts. Greek θ did not shift to Low German d , nor this to H. G. t . The sound of the Parentspeech which in Greek became θ , became in G. T. d . By General Teutonic is meant that Teutonic language which existed before there was any Low German or Gothic or High German. The term Parentspeech is a favorite one of Prof. March.

The formulas run then as follows:

	Parentspeech.		General Teut.		H. G.
I.	x	$>$	y	$>$	z
II.	y	$>$	z	$>$	x
III.	z	$>$	x	$>$	y

The advantage of using $x y z$ in these formulas is, that they are employed as symbols in other sciences and have in themselves no meaning or force. Whenever I wish to apply one formula, say the first, to study the transition of $x > y > z$, to inquire into the causes of the transition, the first thing to do is to find out the sound-value of $x y z$ in the three groups. Scherer, Paul, Sweet and March have insisted upon this non-identity of letters and sounds, and have done much to clear away misunderstandings and hindrances and thus to advance our knowledge upon the subject. I am aware that Mr. Douse, in 'Grimm's Law: a Study' (London, 1876), has used H, S, A , as algebraical symbols, but they played the mischief

with him, because he does not appreciate the importance of the inquiry into the value of his *H*, *S*, *A*. It is wonderful what a damper such an inquiry is upon one's enthusiasm over the beauty, uniqueness and regularity of this famous linguistic phenomenon. When I read such statements of Mr. Douse,¹ "that these tabulations are of identical value, or severally represent precisely the same facts (differing only in order of sequence), will be seen by comparing the vertical columns of any one with those of any other"; or again, "Whatever phonetic operation, as it were, is executed upon the Classical system to produce the L. G. system, must also be executed upon the L. G. to produce the H. G.; and (what is equally important, but is rarely if ever made prominent) the very same operation, when executed upon the H. G. system brings us round again to the Cl. system." Indeed! I should say that to get from H. G. $x = ts$ or pf back to Prsp. $y = d$ or b would require not so much a surgical operation as a somerset or balloon-ascension. I believe Dr. Murray must have been reading just such erroneous statements as these when he wrote those indignant letters to the 'Academy' about two years ago (Feb. 23 and March 2, 1878), in which he went so far as to deny that there was any Grimm's Law. He really meant, I think, Grimm's Law is no such thing as Mr. Douse represents it to be, which is exactly the truth.

The symbols, instead of being a snare, should be a safeguard. Substitute in Formûla I the dental mutes and we have—

$$\text{Prsp. } dh > \text{G. T. } d > \text{H. G. } t.$$

This shifting is beautifully regular. The only drawback about it is, that we are not sure of x , the starting point. The German translator of Mr. Whitney's *Altind. grammatik* says (§ 37), European scholars pronounce Skrt. dh as $dH = d +$ aspiration, nearly as in *kind-heit*. But this is an example for the eye merely, since *kind-heit* is pronounced *kint-heit*, whose medial sounds are rather surd tH than sonant dH . It is clear then, that the pronunciation of European Sanskritists will not help us out. The uncertainty of the acoustic value of Prsp. x is unfortunate, if Formûla I is, as some think, the 'Kernpunkt des Problems der Lautverschiebung,' and if, as is probable, the whole shifting started with it. Paul, in *Paul und Braune's Beiträge*, p. 155, thinks, to be sure, that Arendt has put the existence of 'medial aspirates' out of doubt. But few will agree with him. What discovery did Arendt make? He observed (in 1859)

¹ Quoted from Rhys's review in the *Academy*, 1877, p. 123.

the pronunciation of Said Muhammed and claims that he pronounced a real *dH*, *bH*, *gH*. From this he concludes that twenty modern dialects in India pronounce it so, and then Sanskrit *dH* must have had that sound. Brücke investigated the pronunciation of the same native and observed no medial aspirate. Here were two flatly contradictory results. Which of the two observers was right? Brücke was under this disadvantage, that he had beforehand committed himself to the opinion that a medial aspirate was a physiological impossibility. Scherer and Max Müller, however, sided with Brücke, and Max Müller even tried to come to the rescue with the old Sanskrit grammarians. Curtius and nearly all other philologists accepted Arendt's statement, and in explaining the transitions from Prsp. $x > y$, gave x the value of sonant stop + surd breath, and found nothing easier and more natural than that *dH* should lose the *H* and become *d*. But in 1873 Mr. A. J. Ellis observed the pronunciation of two natives, Messrs. Gupta and Mookerjee, and discovered no sonant stop + surd breath, but sonant stop + "glottal buzz," accompanied "by a momentary energizing of the following vowel." While this was not exactly what Brücke observed, yet it would have told strongly against Arendt, had Ellis's observations been known to anybody in Germany except Sievers. Sievers, in his *Lautphysiologie*, p. 95, expresses the opinion that a sonant stop + surd breath was theoretically impossible and the Skr. medial aspirate must have been sonant stop + sonant breath. Though the *Lautphysiologie* did not come out till 1876, he claims he always held this opinion, and was delighted to see it confirmed by the facts observed by Mr. Ellis.¹ The impossibility then of $x = dH$ or *bH* or *gH* must be admitted, and with that the old theory of the transition of $dH > d$ by the loss of the second element falls to the ground. Curtius, and later Kräuter, have upheld this view. Scherer, and before him Raumer, discarded the medial aspirate and substituted the medial affricate *dʒ* (Brücke's *d'z'*) for *dH*. The transition then was $dʒ > ʒ > d$. This has been more fully established by Paul in his long investigation, *Paul und Braune's Beiträge*, I, 147-201, and may be called the Scherer-Paul theory. But Paul is inclined to start with Prsp. *dH* and then $dʒ > ʒ > d$. He finds it hard to accept with Scherer the primitiveness of medial affricates. But this is not

¹ For a full account of Ellis's and Brücke's observations see Ellis' *Early English Pronunciation*, p. 1134-1137, and Brücke's *Grundzüge*, etc., pages 115 and 116, new edition.

at all necessary. We must start with the true medial aspirate of Sievers and Ellis, which we shall transcribe as d' . This is a double sound, consisting of sonant stop and voice, which is continued after the d explosion until the 'jerked' vowel commences. The series then was $d' > \delta > d$. As soon as the d closure was slackened in the least, the homorganic sonant spirant began, and, as is generally the case, the second continuant element prevailed over the first explosive one. But if, with Scherer and Paul, we retain the medial affricate, the development of that from d' is more natural than from dH . The more stopping-places we can find in the passage of one sound into another, the more time is gained, the less the chances of collision with other sounds. Only the intermediate sounds must not be 'aus der Luft gegriffen,' but must actually occur and must not mislead. The seeming transition, for instance, of Prsp. $t > G. T. d$, in Formula III, has many intermediate sounds, but they are all verified. The transition was $t > tH > tth > th > \delta > d$. tH occurs in Sanskrit and in Southern German dialects; tth in the Irishman's 'thin.' th is the surd spirant, δ the sonant one. Formula I runs nearly parallel to this: $d' > d\delta > \delta > d$. d' occurs in modern Bengalese dialects; $d\delta$ sometimes initially in modern English. Physiologically the transition is as follows: Adding a vowel, we have $d' > a$, in which $>$ indicates Sievers' 'tönenden hauch' after the explosion of d . With the least relaxation of the d closure we have the beginnings of the sonant spirant, which will increase as the first element decreases, detracting also from the energy ('jerk') of the following vowel. Both in surd and sonant affricates the second element encroaches upon the first, as a rule, until the first is lost entirely and a simple sound is the result, as Greek $tH > \theta$, H. G. $pf > f$.

For all three classes of mutes in Formula I, Paul has shown that they reached the sonant stop only in the beginning of the word, and that the guttural sonant spirant appears even there in Oldest Low German and Anglo-Saxon, but that medially the sonant spirant appears except after nasals. Zend, Keltic, Slavic and Lithuanian show the same shifting. In the last two the sonant stop is always reached, whether initially or medially.

A most extraordinary development is that of the Greek surd aspirates and spirants from Prsp. x . It is a difficult question, and I only mention it now, lest I seem to underrate its difficulty. But is not the transition from our value of x made more difficult still? I dare say it is, but the other transition was made easy by starting

with a fictitious value that would best suit the result. We cannot regret that Curtius' plausible explanation in his *Grundzüge*, p. 393, viz: by assimilation of dH, bH, gH to tH, pH, kH (later $> \theta, \varphi, \chi$), falls to the ground. Kuhn, Sonne and others claimed that this transition was a 'strengthening,' which would have been against the main drift of the whole Lautverschiebung, and that kH, tH , etc., must have been the original Prsp. sounds, from which gH , etc., arose by 'weakening.' While it was easy to refute Kuhn's opinion on other grounds, the objection of 'strengthening' seemed to stand, and so Curtius resorted to assimilation. The second element was surd breath, the first was sonant. They must both become surd, hence kH . Now this very incongruity of sonant and surd, which necessitated their assimilation, is one of the reasons for the non-occurrence of any such compound as dH . If d' was the value of x , its transition into tH or th was chiefly a matter of sonancy and surdness. But this question is connected also with another, which even Curtius (p. 84) admits is still an open one, viz: whether there were not surd aspirates by the side of sonant aspirates in the Parentspeech. Grassmann held that such was the case. The Italic correspondents are f for b' and d' , h for Gr. $kH < \text{Prsp. } g'$, and in Latin medially the homorganic sonant stops.

The order of the shiftings I must leave for another time. I have already used so much space for this formula that I must despatch the rest more rapidly.

Formula II with dental mutes becomes—

Prsp. $d > \text{G. T. } t > \text{H. G. } ts, s.$

The value of all of these is fixed. 'Aspirate' does not at all apply to H. G. x , even if it should to Prsp. x . ts is beyond the line of mutes and is a surd affricate. The signs for H. G. x are very numerous, but do not concern us now, and one kind of z is difficult to print.

Formula III becomes—

Prsp. $t > \text{G. T. } th > \text{H. G. } d.$

G. T. th was the surd spirant; H. G. d does not appear regularly. Finally, it was probably surd. Other signs for it are t, th, dh . The labial mutes introduced in our schedule will give us in Formula I—

Prsp. $b' > \text{G. T. } b > \text{H. G. } (p) b.$

To Prsp. b' correspond Greek φ , Latin f and medially b . y is lip-shut-voiced and z lip-shut-voiceless, but the exception rather than the rule, no shifting having taken place.

Formula II becomes—

Prsp. $b > G. T. \phi > H. G. \phi f, f$.

H. G. ϕf , like ts (z), is the double sound, called affricata by some in distinction from the simple spirant or fricative. Other signs are $\phi\phi h, \phi h, v$. $f, \phi h$ and v represent the same sound, viz: lip-teeth-open-voiceless. The second shifting was also shared by the numerous O. H. G. words borrowed from other languages, and such a ϕf or f does not go back to Prsp. y , but z .

Formula III—

Prsp. $z > G. T. x > H. G. y$,

is too large for the labials. There is no shifting from G. T. $f > b$, and it is merely Prsp. $\phi > G. T. f$, particularly when initial. f was originally only surd spirant, but became then sonant medially. The guttural or palatal mutes substituted in the schedule will read in Formula I—

Prsp. $g' > G. T. g > H. G. (k) g$.

To g' correspond Gr. χ , Latin h and g , Sanskrit h as a rule. For H. G. the rule is g , the exception k , hence no shifting. The sign $gh = g$. ch occurs finally in Otfried, and was then back-open-voiceless instead of back-shut-voiced.

Formula II would read—

Prsp. $g > G. T. k > H. G. ch, (k)$

ch may be considered the rule in O. H. G., but now k is more common. ch is back- or front-open-voiceless, according to the vowel immediately near it.

Formula III reads—

Prsp. $k > G. T. h > H. G. h (g)$.

No second shifting is the rule. Initial h in both G. T. and H. G. is surd breath. Like the other G. T. surd spirant, h could become sonant medially and then g (Verner's Law). H. G. medial and final h, hh, ch have the same value as the preceding ch .

Now if this be a correct statement of the principle, several points are clear, which have been frequently covered up by false comparisons and figures of speech. The shifting is not circular, and cannot be compared with the movement of the wheels of a wagon or of the spokes of a wheel, or with three bent arrows pursuing one another in a circle. The varying values that the aspirates assume forbid it, and so does the incomplete shifting of H. G. The process is not 'weakening' or 'lightening of sounds' alone, as some claim.

Call 'weakening' ease of utterance or euphony and we grant that it plays an important part in Grimm's Law, as it does in all phonetic changes. Euphony explains Prsp. $x > G. T. y$, perhaps Prsp. $z > G. T. x$, but surely G. T. surd stop into H. G. surd affricates is no weakening process, for in this Grimm saw, or thought he saw, evidence of the manly, warlike spirit of our ancestors.

Prof. March, in the excellent treatise mentioned below, was the first to give prominence to a tendency so strong in the High Germans of to-day, of unvoicing sonants. It certainly explains the passing of G. T. sonant stop into H. G. surd, and yet this is strictly carried out only in the dental mutes. The passing of Prsp. $y > G. T. z$ is, however, nothing but loss of sonancy. Under this head would also come the development of Greek and Latin surd aspirates and spirants from Prsp. x . Raumer and Scherer are always on the hunt for missing links that are to bridge over the chasm, generally imaginary, between two sounds. Thus they have put between d and t , for instance, 'die geflüsterte Media' as transition sound. Now d is the point-stop-voiced and t is the point-stop-voiceless, and no transition sound is called for. No one single fact or principle has yet been discovered and proposed that will explain all the shiftings of Grimm's Law. I believe none will be found, and it is a mistake to look for one. Foreign influence upon the High Germans was first brought forward by Scherer to explain the second shifting. Prof. March favors this idea. Dr. Murray suggested the influence of the early inhabitants of Southern Germany, upon whom the Germans forced their language. Scherer suggests Romance influence. These are valuable suggestions, and the right direction and principle have been pointed out in which sound results may be obtained. Granted foreign influence upon the dialect of one tribe or district, how would this affect the sister dialects? Such a question cannot be correctly answered until the importance of the study of living dialects upon the old ones is more fully recognized by investigators. The Old High German dialects have lately received much attention from Braune, *Zur Kenntniss des Fränkischen*, and Heinzel, *Niederfränkische Geschäftssprache*.

Much emphasis has been laid by Dr. Murray upon the incompleteness of the H. G. shifting in the letters already referred to. His protests and strictures are indeed called for, and would have had more effect if he had not gone too far in some directions. That the O. H. G. shifting is historical and recent was, it is true, admitted by Grimm, but he liked to lose sight of the fact whenever he wanted

to 'magnify the law.' His framework is much too big for the facts. But is there also a difference in kind between the shiftings as well as in extent?

Most certainly, and while Dr. Murray overlooks the unvoicing of the sonant stops of the Parentspeech and General Teutonic, which is repeated in High German, all before him have overlooked, or at least not appreciated, the differences in the first and second shiftings. The shifting of G. T. $y > \text{H. G. } z$ and that of Prsp. $y > \text{G. T. } z$ are identical. It is the same process—loss of sonancy. This shifting was repeated a third time in the exceptions which Verner's Law accounts for. Prsp. *patar* became first *patHar*, then *fathar*, *faðar*, *fadar*. This G. T. sonant spirant or stop underwent the same fate as the sonant spirant or stop that arose from Prsp. $x (= d')$ and both became surd in H. G. Thus the t of New H. G. 'tun' and 'vater' are of very different origins, yet their last changes were identical, if not contemporary. But the H. G. shifting differs from the G. T. very much in kind. Thus H. G. x , excepting of course non-shifting, is not at all identical with General Teutonic x , though both arose from surd stops. H. G. x is either surd affricate or spirant, G. T. x only surd spirant. Hence the transitions from Prsp. and G. T. z to these sounds cannot be put side by side.

The uniqueness of Grimm's Law has been made more prominent than there is ground for. When we consider that the change of Prsp. x into spirant or stop took place as strictly in Slavic and Lithuanian as in G. T.; that it occurs in Sanskrit, in Keltic and medially in Latin; that the changes are not merely from one group to another, but within the same group and language; that in the labial and guttural mutes (except $k > ch$) there was no shifting in H. G.; that in modern languages we find many parallel transitions, then we shall be less inclined to consider the *lautverschiebung* as such an extraordinary phenomenon. We shall be better disposed and fitted to investigate it from the fruitful side and with proper methods. Any mingling of aesthetics and patriotism with phonology, '*lautphysiologie*' and dialect-study, within whose spheres the phenomena fall, is entirely unscientific. We can pardon it in Grimm in the enthusiasm of discovery, and in Scherer, who claims the prerogative of Lessing's genius as an excuse for his mistakes, which, he is sure, will lead others on the right track.

We come now to the exceptions. Onomatopoeic words remain unchanged from their very nature. The surd mutes p , k , t , in

close contact with preceding spirants cannot shift, hence *st*, *sk*, *sp*, *ht*, *ft*, are unchangeable. *d* is sometimes protected by *n* and *l*. In 1862 Lottner made a very careful examination and collocation of the exceptions to the first shifting in Kuhn's *Zeitsch.* XI, 161-205. Besides those above mentioned he found two classes of exceptions. The first is in Formula I. In them Gothic *y* (sonant stops) appeared to correspond to Skt. *y*, and there was no shifting apparently, *e. g.* Gothic *dauhtar* = Skt. *duhitar*. The second is or was in Formula III, in which both Gothic sonant stops and surd or sonant spirants corresponded to Skt. surd stops, *e. g.* Gothic *bairand* = Skt. *bharanti* (*d* = *t*) and Gothic *fadi* = Skt. *pati*. The first class had been allowed to pass partly because it was considered quite natural that there should be exceptions, and they proved the rule, partly because the Sanskrit forms were supposed to be identical with the Parentspeech forms. Grimm always put some one of the Indo-European family of languages as the first member of his schedule. It was Grassmann's great merit to have proved in his article 'Ueber das ursprüngliche Vorhandensein von Wurzeln, deren anlaut und auslaut eine aspirate enthielt' (Kuhn's *Zeitsch.* XII, p. 110-138), that Sanskrit was not primitive in this case, that the Parentspeech had had a 'medial aspirate,' if that be the value of Prsp. *x*, and that hence the exceptions to the first class were not exceptions at all. We had not started with the right Prsp. letter, but with the Sanskrit or Latin or Slavic. In other words, there was a shifting of *x* > *y*, which was so general as to extend over Sanskrit, Greek and Latin partially, over Slavonic, Keltic and Teutonic wholly. Grassmann demonstrated this within a year after Lottner's article appeared.

The second class of exceptions, that of the G. T. double correspondences (sonant stop and spirant) to Prsp. surd stop, waited for an explanation much longer. That the shifting was not directly from *t* to *d*, for instance, was recognized already by Raumer in 1837; again maintained by Scherer (*Zur Gesch. der deutschen Sprache*), and by Paul (*Zur Lautverschiebung*) in Paul und Braune's *Beiträge* I, 147-201. The transition is now generally put down and accepted as follows: *t* > *tH* > *th* > *ð* > *d*. The first part, *t* through *tH* into *th*, is the regular shifting. The surd stop became first dental aspirate in the strict sense, *t* + *H*. The aspirate became surd spirant. The exception lies in this, that it did not remain surd, but became sonant medially, and then the sonant stop. How is this change to be accounted for? Verner discovered the reason.

It lies in the accent. Mr. Sweet framed a very bold theory in his edition of Gregory's *Pastoral Care*, published by the Early English Text Society, in which he puts down the following series: Prsp. $t >$ oldest Teutonic $d >$ oldest Low German $dh >$ oldest H. G. d . The primitiveness of d is based chiefly on the frequent and easy interchange of d and dh . Prof. March, in an article 'On Recent Discussions of Grimm's Law,' Transactions of the American Philological Association, 1873, objected very strongly and effectively, and if he did not overthrow Sweet's position, Verner's Law certainly did, as Mr. Sweet handsomely acknowledges in a letter to the 'Academy,' February 9, 1878. Paul, in the article referred to, though the subject of his investigations is really Formula I, has much to say about Formula III and its exceptions. He tries to make out, that medial b, d, g , are really sonant spirants in Gothic (p. 151); that the only difference between medial d and th is that d is sonant spirant and th the surd one. And one of the reasons adduced is also the easy interchange of b, d, g , with the corresponding spirants. Both Sweet's and Paul's theories are indirect unsuccessful attempts at explaining the exceptions of Formula III. Though Paul's idea of the manner of the transition is perfectly correct, of the cause he had no idea.

I shall try now to state and illustrate Verner's Law as briefly and clearly as I may. His own statement of it is this:

"Indogerm. k, t, p , gingen erst überall in h, th, f , über; die so entstandenen tonlosen fricativae nebst der vom Indogerm. ererbten tonlosen fricativa s wurden weiter inlautend bei tönender nachbarschaft selbst tönend, erhielten sich aber als tonlose im nachlaute betonter silben." Paul, in P. und B. Beitr. V, 538, restates it in this way: "Die nach vollzug der germanischen verschiebung vorhandenen vier harten reibelaute h, th, f, s sind ausser in den verbindungen $ht, hs, ft, st, sk, sp, ss$, erweicht, wenn der nächstvorhergehende sonant nicht nach der ursprünglichen betonung den hauptton trug." Omitting s at first, it would read in this way: Prsp. k, t, p , shifted without exception to h, th, f . But these became medially in unaccented syllables sonant spirants, except in certain surd consonant-combinations. This is sufficient, if every medial b, d, g , in Gothic or Ags. are sonant spirants. But if not, we must add "and these sonant stops"; s , though not within the limits of Grimm's Law, joins the other spirants. Then it may be stated as follows: Medial G. T. h, th, f, s become the sonant spirants gh, dh, v, z , in unaccented syllables, and later the sonant stops g, d, b , and r (= point-open-voiced).

A fictitious word *asataka*, after Verner's *akasatam*, would take these shapes in General Teut., Gothic and H. G., having passed through *asatHakHa* and *asathakha*, according to the primitive accent:

G. T. ásaðagha-, azáthagha-, azaðákha-, azaðaghá-,
 Goth. ásadaga-, azáthaga-, azadákha-, azadagá-,
 H. G. ásataga-, arádaga-, aratákha-, aratagá-.

As examples of actual words these will suffice:

Skt. bhrátar	G. T. brōthar,
but " pitár	" fadar,
and " mātár	" mōdar;
" ántara	" anthara,
but " antár	" undar;
" çvácúra	" swehra,
but " çvaçrú	" swegrā.

Skt. pf. ind. sing. bibhédā, bibhéditha, bibhédā =

G. T. pret. ind. sing. laith, laist, laith (not etymologically of course); but

Skt. pf. ind. pl. bibhidimā, bibhidā, bibhidās =

G. T. pret. ind. pl. lidum, liduth, lidun.

Skt. causatives have the accent upon the ending, *bhārāya*, which shows itself in G. T. causatives, *e. g.* G. T. *hangjan* from *hanhan*, *laidjan* from *lithan*, *nazjan* from *nesan*.

The comparative is interesting. Since there is a retraction of the accent, as in ῥόβ-, ῥδιον, ῥδιστος, when it rests in the positive upon the second syllable, the G. T. ending must always be *-izan* and *-ozan*, later *-iro* and *-oro*, *e. g.* *batizan*, Ags. *betra*; *blindozan*, O. H. G. *blindoro*.

It would be interesting to trace Verner's investigation from the beginning and see how he was gradually led up to the discovery. We must be satisfied with reproducing here an equation from p. 109:

G. T. $\frac{\text{tehan}}{\text{tegu}} = \frac{\text{slahana (inf.)}}{\text{slagana (p. p.)}} = \frac{\text{brōthar}}{\text{mōdar}} = \frac{\text{kwehthana}}{\text{kwedana}} = \frac{\text{mūsi}}{\text{deuza}} = \frac{\text{keušana}}{\text{kuzana}}$

He reasoned correctly that an explanation of one of these must be an explanation of all. He found the clew in the second, fourth and sixth fractions, which illustrate that remarkable phenomenon called 'grammatische wechsel' in O. H. G. and M. H. G. grammar. The last one who wrote upon this subject before Verner was Braune, P. und B. Beitr. I, 513. Braune says that hitherto we have been satisfied with merely stating the fact of the change, without showing the inner connection between these separate phenomena. Paul's

theory of the origin of sonant stops and sonant spirants proves this inner connection, as he will proceed to show more fully. In fact, Braune sees in the 'grammatische wechsel' proof of Paul's theory. But when he begins to realize fully that Gothic especially and old Saxon and Frisian do not follow suit, he says: "Ueberhaupt aber muss man das wol beachten, dass dieser ganze lautwandel nicht auf einem streng-durgeführten lautgesetz, sondern nur auf einer sehr ausgeprägten lautneigung beruht." This distinction between a law and an 'inclination' would never do for a '*junggrammatiker*.' The trouble was with Gothic. It showed very slight traces of grammatische wechsel. Its primitiveness was not questioned on this point any more than that of Formula I. Verner's Law was not found, because it was not looked for, and the facts were not so properly and comprehensively grouped as in the above equation, in spite of Paul's and Braune's constant efforts and frequent publications. Scherer, who if he cannot solve a phonetic problem phonetically will resort to aesthetics and 'sprachgeist' and 'männische und weibliche periode' in literature and language, singled out *fadar* and *modar*, and accounted for their *d* because they were "more frequently used words than *brothar*." *Fadar* and *modar* must belong to the 'small coin of language,' and must have been used so much that they are worn smooth and have nearly disappeared in Gothic. *Modar* never occurs, only *aithei*; *fadar* only once, in its stead *atta*, while *brothar* has no synonym at all. No; the accent explains either all members of the above equations or none. *Slahana* inf. has the primitive accent on the stem, *slagana* the past-part. on the suffix. The same is true of *quethana* and *keusana*. The pret. sing. was accented on the stem, the pret. plur. on the suffix. The 'grammatische wechsel' is a part of that so-called great exception to the first shifting. It is that general Teutonic shifting of surd spirants to sonant ones and sonant stops in unaccented syllables. If one dialect, whether old like Gothic or young like modern English, does not conform to this law, what seems an apparent exception must be accounted for in some way or other by tendencies and phonetic principles within that individual dialect. Gothic, for instance, must have once had grammatische wechsel. It traces of it now, e. g. *aih-aigum*, *tharf-thaurbum*, *filhan-fulgins*. There is a tendency in all languages, and very strong in Gothic, called 'ausgleichung' by German scholars, levelling or striving after uniformity. In Gothic it manifests itself clearly in the reintroduction of *i* and *u* for *e* and *o* in G. T. and the sister dialects, which gives it that appearance of primitiveness. Thus it has made the

pret. pl. and past-part. again like pres. and pret. sing., and *z* in declension again *s*. But *z* has never become *r*, as in the other dialects. In Goth. we have *tiuhan*, *tauh*, *tauhum*, *tauhaus*; *h* is uniformly restored. In Ags. we have *tiohan*, *toh*, *tugun*, *togan*; in N. H. G. *ziehen*, *zog*, *gezogen*, which is more ancient on this point than Gothic. Again, in N. H. G. *zeihen*, *zieh*, *geziehen*, we have uniformity, as in the cognate Goth. *-teihan*, *-taih*, *-taihans*. The N. H. G. class of verbs, *schneiden*, *schnitt*, *geschnitten*; *leiden*, *litt*, *gelitten*; *sieden*, *sott*, *gesotten*, is easily brought into line. The G. T. forms with the spirant became the H. G. forms with *d*, and those with *d* received in H. G. *t*, for instance, Angs. *snidhan*, *snadh*, *snidon*, *sniden* = O. H. G. *snīdan*, *sneid*, *snitum*, *snitan*. With the H. G. tendency to make a final sonant stop surd, and the good sense of the writers, especially of M. H. G., to spell as they pronounced, *leid* and *sneid* became *leit* and *sneit*, and then in N. H. G. *litt* and *schnitt*, the vowel difference between pret. sing. and plur. having soon disappeared.

But how and why did the accent produce such a remarkable effect? Admitted the tendency of any surd spirant to become sonant when surrounded by sonants—which is nothing but leveling—the question is, how did the accent preserve the surd character? Verner explains it in this way. The G. T. accent must have been one of stress also, and not of pitch merely, which was the primitive accent. The strong impulse of surd breath, when the stress is thrown upon the syllable with the surd spirant, would preserve this spirant as it was originally. Let the accent be thrown upon any other syllable, and the surd spirant will weaken from a fortis to a lenis, and then become sonant in sonant surroundings. (See Sievers' Lautphysiologie, p. 133.) I dare say, many will be satisfied to know that the preservation of the surd spirants *h*, *th*, *f*, *s*, or their transition to sonant spirants and to sonant stops, goes hand-in-hand with the original accent. Verner's Law settles also a point in the history of accent. The free Parentspeech accent was still preserved in G. T. after the beginning of the first shifting of mutes. When the characteristic Teutonic accent, which is limited to the stem-syllable, commenced, the shifting of spirants was accomplished, or at least so well under way that the new accent had no influence upon it. Had the G. T. logical accent been as old as was formerly supposed, it would have prevented this shifting.

Verner's Law strengthens the position of the 'junggrammatische schule,' claiming that phonetic laws admit of no arbitrary excep-

tions. They must and can be applied as strictly as physical laws, if they are laws at all. By Verner's discovery the last large class of exceptions to the first shifting in Grimm's Law has been explained. They have been proved not to be real exceptions. Prsp. *z* became G. T. *x* initially, medially and finally. But medially they shifted within the same group of languages a second time and a third time in H. G., at least in the dental mutes. Verner's Law has lately been studied and cleared up very much. See Fr. Kluge, Beiträge zur Geschichte der germ. Conjugation; Paul, Zum Verner'schen Gesetz, P. und B. Beitr. VI, 538. Sievers, P. und B. Beitr. V, 149, has already found a corollary: *g* (*gh*) disappeared between originally unaccented vowel and *w* already in General Teutonic. In Gothic we have uniformly *h* in *saihwan* (G. T. *sehwan*), *sahw*, *sehwum*, *saihwans*, but Ags. shows the older forms, *seon* (for *sehon*), *seah*, *sāwon*, *gesewen*, where *g* from *h* is lost before *w* in the pret. plur. and past participle, though there are forms which still show it.

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